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DR. PAINE'S MEDICAL AND PHYSIOLOGICAL COMMENTARIES.

[Continued from page 54.]

We now return to the subject of the anatomical characteristics of typhus, typhoid affection, or continued fever. The reader will bear in mind that anatomical characteristics are very different from the causes of disease. The anatomical characteristics are, comparatively speaking, easily discovered by careful examination—and in common, but somewhat inaccurate language, we allow they are called the causes. For instance, Louis says inflammation of the lung is the cause of the various groups of symptoms usually accompanying and giving us information of the existence of that disease. But there is a step still farther back, and which is hidden, for the most part, and to this step must we go for the real causes. These are the most hidden subjects we have to deal with. In the present age, and with our actual knowledge of the phenomena of life, it will be long ere we shall be able to *know* much upon etiology—yet upon nothing do medical authors “tax their imagination for their facts” more than upon this same topic.

The anatomical characteristics of typhus, according to Louis, are of two kinds, *primary* and *secondary* (a similar opinion to Chomel's). In the first rank, Louis places the affection of the congregated follicles (Peyer's patches), because “they were more or less seriously changed in structure in all the patients.”\* But there are other *secondary* characteristics—“Ulcerations of the pharynx and œsophagus having occurred only in a small number of typhoid patients, and in no other disease, may be considered as among the anatomical characteristics of the former, though they are secondary.”† Now we cannot see how Louis could have drawn any other inference. We beg the reader to remember that Louis's work was written from cases collected some time ago, between 1822 and '27, and that he pretends “not to give a perfect treatise on the disease.”‡ He examines his own cases collected in Paris, and in wards devoted to adults. Doubtless, he never intended to say that there might be no difference observed between the typhus of Paris and that of England or America. He left that for others and future observers to decide. Doubtless, moreover, he would have made a more perfect work had he studied the disease in children and old age, and in other countries. He would have had a larger number of facts. But supposing he had devoted 6 years to children, 6 to the aged, 6 to England,

\* Louis on Typhoid Fever, Vol. 1, p. 172. † Ibid., p. 302. ‡ Preface to Typhoid Fever.

&c. ; his life would have terminated, and we should not have had the "model" work by M. Louis. But ought we to complain of a man who makes inferences from the facts he has in his possession?

Dr. Paine states (p. 169) that the follicular affection is denied to exist by Louis and his followers in genuine typhus. We know not to whom Dr. P. refers under the title of followers; but as it regards Louis, we deny that he ever made such an assertion, and challenge Dr. P. to produce any proof to that effect.

Dr. P. quotes from Lombard and several English and American writers in regard to the characteristics of fever. Even if it be certain that in the course of fever in England and America there are other lesions than those described by Louis, we can derive no argument from this fact against Louis's results—inasmuch as he limits these results to the disease as actually existing in Paris. The subject, in our opinion, is still sub judice as it regards these characteristics, when fever is viewed as an affection liable to occur in every part of the world; but this does not lessen our confidence in M. Louis; and though we may differ from him, we may respect his method of investigating, believing, as we do, that some higher law will eventually reconcile all differences. But at present, we must conclude, both from Chomel's and Louis's researches (82 fatal cases collected during a space of 12 years by two eminent men), that the anatomical characteristics of typhus in Paris are as have been described. But Dr. Paine has no right to use such an expression as this, when criticizing Louis's results. "They are," says Dr. P., "designed for every climate, constitution, habits, and other predisposing and exciting causes." As much right have we to complain of Mons. Louis for having said that ulceration of the intestines in chronic diseases is never found except in combination with tubercles in the lungs. Now we have seen two cases, at least, and if we had resided farther south should probably have seen many more, of chronic diarrhœa from long residence in the West Indies; and in these cases, in which we examined the lungs with the utmost caution, making the minute subdivisions of them with especial reference to Louis's remarks, we found no trace of tubercle, but the intestines were studded with ulcers. We explain the fact of the error of Louis from this circumstance, that the diseases of warm climates rarely, if ever, appear in the Paris hospitals. We never saw one during our residence in that metropolis. We do not, however, consider Louis as being unworthy of confidence because he has chosen to say—"It is now more than 8 years since my Researches [on phthisis] were published, and I have not met, during that period, with a single subject who has died of a chronic disease, and with ulcers in the small intestines, in whom there was not at the same time tubercles in the lungs."<sup>2</sup>

But the climax is coming. We cannot forbear smiling to view the overweening self-complacency of our commentator when commencing his plan of "rapidly glancing at the prolific results of those 50 cases of typhus." Observe the bathos! "But it is indispensable to the success

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\* Examen de l'Examen de Broussais. Paris: 1834. P. 18.

of our enterprise, and when we shall have brought them to the solemn consideration of the reader, yet leaving them mainly to his intelligence, we cannot but think that they will be regarded as a fearful beacon to the present and coming generations"! (Page 694.) Let us see how Dr. Paine has succeeded in proving that he deserves immortality for having proved that Louis is an arch traitor to truth, and that his works ought to be regarded as a warning to all future generations!

In the first paragraph after this flourish of "solemn" trumpets, we find a radical error in the statement of Louis's opinions—as follows. "Our author, for instance, has no conception of disease which he cannot trace out through some lesion of structure; and when he endeavors to insinuate the belief that diarrhœa cannot exist 'without appreciable lesion of the intestinal mucous membrane,' he fears that his hypothesis may find some opposition from analogies supplied by the natural conditions of the body." (P. 695.)

Our readers would scarcely believe us if we were merely to state that all this assertion by Dr. Paine is radically false, and that in the above paragraph in which Dr. P. *pretends* to quote from Louis's work on phthisis, Louis really says exactly the reverse of what Dr. P. states that he does, and that instead "of having no conception of disease which he cannot trace out through some lesion of structure," he declares, in this identical paragraph, that he *does* believe in disease of function without appreciable alteration of structure. Dr. Paine may hope to escape the imputation of falsehood, by using the word "insinuate." But let any candid reader read the sentence which Dr. Paine has written, and he would say that Louis really believed that diarrhœa could not occur without lesion of structure, but that he was afraid to say it openly, and therefore merely "insinuated" it. We must say that we feel indignant when such accusations are made against this author, for they show either wilful blindness or total ignorance on the part of the accuser. But let us see the original and translation.

"Observons que ces sueurs si copieuses indiquent un derangement des fonctions de la peau, aussi remarquable par son degré que par sa durée; que ce derangement, qu'il soit sympathique ou dû à une autre cause, n'en est pas moins réel, et a lieu sans alteration sensible de la structure de l'organe qui en est le siege; qu'ainsi qu'une fonction peut être plus ou moins alterée pendant long temps, sans que l'organe qui en est chargé offre de changement appreciable dans sa texture. Remarquons, encore, qu'à défaut de faits qui prouvassent d'une maniere directe que le devoiement peut avoir lieu sans lesion appreciable de la membrane muqueuse de l'intestine, cela serait à présumer à raison de l'analogie qui existe entre des sueurs copieuses et une diarrhœa plus ou moins forte. Nous ne disons pas evident, parceque, dans notre maniere de voir l'analogie ne peut servir qu'à indiquer de nouvelles recherches, à aller à la rencontre des faits, et jamais à les suplier—autrement, ce seroit conclure de la possibilité d'une chose à son existence, ce qui est absurde."—(*Researches on Phthisis*, s. 259.) "We remark that these copious perspirations indicate a derangement in the functions of the skin, as remarkable in degree as in duration; that this derangement, whether it be the

result of sympathy or of any other cause, is not the less real on that account, and occurs without any appreciable alteration in the structure of the organ in which this derangement occurs. And thus we find that a function may be more or less seriously altered for a long while, and at the same time the organ, which is the origin of the function, may present no appreciable change of structure. We would likewise observe, that in the absence of facts which directly prove that diarrhœa may occur without any appreciable lesion in the mucous membrane of the intestine, we might presume that to be true in consequence of the analogy which exists between copious perspiration and severe diarrhœa. We do not say that this is proved (evident, Fr.), because we think that analogy serves only to point the way to new researches; it teaches to seek, in a certain direction, for new facts, but it never supplies the want of them; for if it were otherwise, we should deduce the absolute existence of a thing from the simple possibility of such existence, which is an absurdity."

The inference we draw from the above perversion of Louis's words, is one of these: 1st, Dr. P. has wilfully falsified a remark of an author whom he wishes to hold up to scorn; or, 2d, Dr. P. reads so carelessly that he did not observe his mistake. Upon whichever horn of the dilemma the Dr. may place himself, the inferences are not very pleasant for a man who writes such full commentaries upon the medical theories of the day. But our commentator may not choose to think himself placed as we think he is. Be it so, and let us hear him in his future remarks. After a long defence of analogy as a source of evidence, he concludes thus triumphantly! "Thus in the example which our author fears may encroach upon the dominion of morbid anatomy, who is there that will not concede that 'profuse perspiration' arising from disease without 'any appreciable lesion' of the skin, is not a substantial ground for induction that 'diarrhœa'—aye, and many other morbid results—may take place independently of any 'appreciable lesion' of structure? And to show you [mark well the Dr.'s earnestness] how analogy may grow into a matter of fact, and in this very instance, we will point you to serous effusions in the brain, thorax, abdomen, where the secreting membranes often exhibit their perfectly normal state."—(P. 695.) Heaven defend medical art from the "*facts*" which grow up in this way. A man has sweated, and no change of structure of skin is observed—therefore, says Dr. P., we are certain that a man may have hydrothorax, dropsy of the brain, hydrocele, &c. &c., without evident change of structure in the organs implicated. We must say that we should prefer to examine the chest and head and see whether these things are so, rather than to infer that these diseases exist merely from what passes upon the skin. It is a long while since we studied logic under the venerable Dr. H. Would that we could appeal to that learned man. Even he, with all his logical acumen, would be shocked at such unwonted use of analogical reasoning.

We hasten to another instance of our commentator's unfairness. On page 696, he says—"Our author has no difficulty with analogy where a lesion of structure may embellish the philosophy of disease. Thus:

'Analogy,' he says, 'is in favor of what we advance. For, when hæmorrhage occurs in any internal organ, it is almost constantly a symptom of more or less considerable alteration of structure.' From this assumption, he reasons analogically that 'hæmoptysis (with certain exceptions), whenever it occurs, renders tubercles in the lungs infinitely probable.'

So much for Dr. Paine's assertions. Let us see how the matter really stands. Louis commences the paragraph upon hæmoptysis (Phthisis, s. 231) by stating that 57 out of 87 patients had it. He then states, 25 had it copiously. Again he asks (s. 233), "Are we, however, to consider the hæmoptysis, especially when copious, which precedes cough and expectoration, as a precursor of tubercles, or simply as a symptom which reveals their presence?" He then states, that for nearly three years he had constantly asked all his patients in reference to this symptom, and he found, that except in the phthisical patients, and those who had received injuries of the chest, or in whom the catamenia were disordered, none had hæmoptysis. And he continues thus: "We think, therefore, that hæmoptysis, except in the cases above mentioned, at whatever period it may occur, makes it infinitely probable that tubercles exist in the lungs. We do not say that this is certain, because there have been many well-attested facts which are fortunate exceptions." Moreover (he continues, in the next paragraph), "analogy favors this proposition. For when a hæmorrhage," &c. And he terminates the paragraph thus: "But let us cease with these few remarks [that is, reasonings from analogy] which are intended much less to supply facts, than to excite to investigation."

Really, if Dr. P. did not put forth such pretensions to learning and candor, we might apply to him much harsher epithets than those we have already used. Does not that man deserve severe rebuke, when under pretence of stating an argument in reference to a subject, he dares entirely to reverse the order, and uses a remark made by the author for the purpose of exciting others to investigate, as if it were the chief corner stone of the author's argument? We heartily detest such trickery.

Our commentator seems to be unwilling that M. Louis should dare use the word *experience*, unless he has numbers to prove it—so bigoted does he seem to suppose our author is in regard to the numerical method. For instance—on p. 699, we find, "Here is another example of adherence to 'rigorous facts,' and of the uses which our author makes of analogy, where questions of the most vital and general nature are concerned. Thus: 'Experience shows us, that in spite of these striking and indisputable differences between persons most resembling one another, 999 out of 1000 who differ in age, sex, temperament, &c., live on the same food, prepared in the same manner.'—(*Bloodletting*, p. 58.)"

On page 700, the commentator quotes several passages. In reference to one, he says that Louis regards structural disease as the essential pathology of disease. We have already denied this assertion; but we quote the following: "If the senses do not appreciate everything, if there is anything else in the typhoid affection than what the eyesight can

discover, such is also the case in almost all internal diseases, which in this respect " (in having something more than *structural lesion*) " are scarcely less mysterious than fevers."—(Page 394, *Typhoid*, Vol. 2.) Our commentator returns to the charge afterwards (Vol. 2, p. 762), and quotes various passages from Louis's works, tending to prove (as Dr. P. thinks) that Louis regarded the lesion of Peyer's patches as the *cause* of the symptoms in typhus. Now the sum and substance of the whole of the quotations may be illustrated thus. A case of very severe pneumonia occurs, accompanied with delirium. Death ensues—chiefly, as Louis thinks, in consequence of the mania. Yet upon examination, we find both lungs extensively hepatized, but no appreciable change in the brain. Louis says, that in the present state of our knowledge we must refer the mania to the disease of the lungs, and not to any material change in the brain. Now it really seems to us that Louis is correct, though we see no sufficient reason for his urging the point so much. We have no doubt that no delirium would have existed had not the man been affected with pneumonia. Just so does Louis regard the delirium in typhoid fever; believing, as he does, that the lesion of the alimentary canal is as characteristic of the typhoid disease, as hepatization is of pneumonitis. In one sense, pneumonia was the *cause* of delirium—yet how different our ideas of causation when it is regarded in this light, from those previous causes which give origin to the whole phenomena of disease; and upon these, hear what Louis says. "The deepest obscurity hangs over the causes of the affection under consideration."—(Vol. 2, p. 393.)

Below we have another specimen of our commentator's unfairness, with either a disposition to lead the reader astray, or great carelessness in quotations. After quoting from the *Typhoid Fever* (Vol. 1, p. 152), "that Louis thinks that the symptoms of disease of the stomach are very obscure"—and in another place "that the mucous membrane of the stomach was more or less seriously altered in the greater proportion of cases," Dr. Paine triumphantly compares them with another passage (Vol. 2, p. 131-2), and thinks he has discovered an inconsistency between it and the two previous ones. This sentence is as follows: "It is nearly correct to state that the apparent condition of the brain cannot explain the symptoms of which it is the source, any more than the mucous membrane of the stomach can account for the anorexia and other gastric symptoms in the great majority of cases." Now the reader may think that there is inconsistency. Let him read the following, from Vol. 2, p. 39. "Thus we see that out of 30 subjects from whom I was able to learn anything about the gastric symptoms, twenty had vomiting, nausea, or pains in the epigastrium, and out of these only eleven had any serious alteration of the mucous membrane of the stomach"—in other words, a proportion of one half had symptoms, but no corresponding lesion. Dr. Paine should have quoted this passage, and not have brought together two sentences upon entirely different subjects. But this, as we have already frequently seen, is but too often the course pursued by Dr. Paine.

We are almost fatigued with the numerous instances of unfairness—

but the following extract affords another specimen. Louis, from examination of the heart, thought it was not inflamed, and he describes it thus. "*At the same time that it was softened, it had less color than usual in many cases. It was of an onion-peel color, which varied in intensity, and (was generally livid and purplish on its surface as in its substance. The internal face of the ventricles and auricles was, on the contrary, of a deep violet red color) which color sometimes penetrated beyond the lining membrane, and appeared owing to an imbibition of blood, which it resembled more or less in color.*"\* Afterwards he states that the walls were thinner than usual. Hence he concludes that inflammation did not cause this affection. But Dr. P. quotes the part of the sentence only that is included in the above parenthesis, and when Louis says "that if we know any cause of disease directly the reverse of inflammation, it would be proper to refer this softening to it," the commentator says "our author refers to the absence of pus in the walls of the heart, as a special proof of the foregoing doctrine;" whereas Louis uses this fact, and likewise the non-existence of pericarditis in any of the cases, as merely considerations to support, in some measure, his previous arguments, and which to any fair mind are sufficient. But, as we have seen in the case of analogy, Dr. P. takes what Louis uses as merely supplementary to the main argument, and puts it forth as the chief groundwork.

Here is another specimen of the misstatements by our commentator. We are sorry to be obliged to use such terms towards a medical associate, but nevertheless the truth must be told in this case at least, let it be never so pungent. Dr. Paine (p. 703) attempts to convict our author of something worse than inconsistency in stating that softening, thickening and ulceration may occur without inflammation, when tubercles exist in the alimentary canal; and in order to gain this end, Dr. P. makes these remarks. 1st, His inductions are *founded wholly upon the debris of the body*; 2d, The inductions rest chiefly upon the fact that the foregoing alterations in structure are white in one case and red in the other. And after two or three pages of quotations from Louis's writings, in which additions in the way of comments and subtractions are made to suit the fancy of the commentator, he finishes with a quotation from Cowper's Conversation, and applies it to the "Numerical Method."

"Such continual zigzags in a book,  
Such drunken reelings, have an awkward look,  
And I had rather creep to what is true,  
Than rove and stagger with no mark in view."

"This may be very philosophical;" but let us see *how* our commentator has convicted Louis of inconsistency on this subject. In regard to the first statement above, I would ask how, *a priori*, without "examination of the debris of the body," can we determine upon the existence of inflammation of an organ under the skin? How originally did we arrive at the idea of hepatized lung, except by post-mortem appearances, or, as Dr. P. says, "from the debris of the body?" The symptoms might

\* Italics our own, in order to mark what Dr. Paine suppressed.

lead us to infer the phenomena of inflammation, but we never could do so unless from previous study of the parts in an inflamed state, and a comparison of this state with the symptoms. So much for this; and with regard to the second, we join issue entirely, and declare that Louis never did propose to decide that a part was inflamed from the existence of redness merely. Moreover, as we deny the premises, so we deny all the inferences drawn therefrom in reference to the credibility of Louis; for we beg the reader to observe that Dr. Paine accuses our author "of occasional guarded contradictions," for the purpose of gaining "a reputation for candor," as "that more effectually secures to him a successful propagation of his favorite, though conflicting hypothesis."

This subject will occupy considerable space. Let us first quote from the commentator, and afterwards compare with it Louis's remarks upon the same subject at the latest period of his life, viz., when writing his work on the typhoid disease.

"Here we pause to consider how far our author has supplied any ground for his principle that softening, thickening and ulceration, of different tissues, sometimes depend upon inflammation, and at other times on an 'exactly opposite condition of disease,' and what, also, is the probable motive for introducing this confusion into the most important branch of pathology.

"In the first place, the inductions are founded *wholly upon the debris of the body*. There is no where, that we have been able to discover, any essential reference to the phenomena of the disease during its actual existence. Even the remarkable similarity of those phenomena appears not to have been held in consideration, in forming the conclusions. Secondly, the inductions rest chiefly upon the fact that the foregoing alterations of structure are *white* in one case, and *red* in the other.—(P. 548, *Andral*.) This may be very philosophical; but let us see what our author thinks of it when he is engaged in reasoning the reader into his problem, and in supplying the appearance of an impartiality which never fails of a prepossessing influence, and carries us along with greater confidence to the never-failing act of generalization. But we have even more than this—a direct contradiction of his own philosophy as it respects the *very important* tests of color, by which our author comes at last at the conclusion that the foregoing lesions are owing in 'the typhoid fever,' at different times, to exactly opposite pathological conditions. Thus: '*Paleness of inflamed structures takes place sooner or later, as is exemplified in the various shades of color of hepatized lung.*' 'It ought to be noticed, that continuous with a red and softened portion of mucous membrane, we often find another equally softened, but *without redness*. *If the first, therefore, is inflammatory, it is probable that the other is also.*' Here, too, he allows that 'thickening of the sub-mucous cellular tissue' was 'an evident result of inflammation,' although 'recent' and 'retaining its natural paleness.' Whereupon, our author lays down a rule which it was convenient to abandon in expounding the lesions of 'the typhoid affection.' Thus: 'This fact [the foregoing], with *many others*, shows that the thickness of our tissues is one of the most important circumstances to be noticed, and

that to *confine ourselves* to the description of the color of membranes is often useless and even a cause of error to those who might draw conclusions from imperfectly described facts.\* And again, in his *Preface*, 'redness, considered by itself, offers much less interest,' than 'thickening, softening,' &c.

"Such was the opinion of our author when reasoning abstractedly upon the results of inflammation, in his work on Phthisis. But, he was also simultaneously engaged about 'the typhoid affection;' and hence we have in the work on Phthisis some ambiguous conclusions as to the dependence of the foregoing lesions of structure upon 'exactly opposite conditions of disease,' as their color might happen to be *red* or *white*. When, however, we come to the work on Typhus, the obscurity is cleared up; and, in a general sense we are told that *red* and *white* must be taken as the ground of an absolute distinction between the pathological causes of such lesions of structure as may be otherwise in all respects alike, and characterized by the same vital phenomena. Nevertheless, it was important to attempt a consistency of doctrine with what had been laid down in the work on Phthisis; and this could readily be done by those occasional guarded contradictions which give to an author a reputation for candor that more effectually secures to him a successful propagation of his favorite, though conflicting hypothesis. Having said this, we are now bound to cite an instance in illustration; and this we do the more readily, as it exhibits, in connection with what we have hitherto quoted from our author, the true foundation of the new philosophy in respect to inflammation and its products, and explains how far morbid anatomy, and specific objects, have been the source of certain existing collisions with the fundamental laws of nature. Thus, then, our author: 'I refer to the sub-mucous membrane of the large intestine, which was very firm, and at least *six times thicker* than natural, and of a *whitish* color. This *thickening*, we cannot doubt, was consequent upon an inflammation of the mucous membrane, but not recent, for the *white* color is inconsistent with the idea of *acute* inflammation.† The reader should here regard in their proper connection and involution, the expressions, 'but not recent,' and 'acute inflammation.'"

Let us now see the reverse of Dr. Paine's picture. Under the title of "Consistence of the mucous membrane of small intestine," Louis says:‡

"But what was the nature of this softening? We can solve this question only by comparing together the thickness, the consistence, and the color of the mucous membrane, about which we are now treating. Let us now examine the elements of this question thus brought into comparison with each other.

"In 9 of the 12 cases in which the softening existed to a greater or less degree through the whole track of the intestine, the mucous membrane was pale or greyish; it was more or less red in the others, at the end or in the latter half, or through the whole extent of the ileum, and the softening was not greater in the latter cases than in the former. There was manifest thickening in two cases only, in which the mucous membrane was white, or had merely some pale red spots in some points.

\* On Phthisis, sec. 135, 136.

† On Typhoid Fever, Vol. II., page 260.

‡ Ibid., Vol. I., p. 170.

What deduction shall we make from these facts? Must we admit that the white and red softening have each their own causes, the one wholly different from the other? that one is of an inflammatory and the other of a different nature? This question, which I stated in another work (*Phthisis*), without being able to decide it, seems to me may now be decided affirmatively, at least, in certain cases. For if it is true that softening is the ordinary effect of acute inflammation, and that, when we find redness, thickening and softening combined, inflammation is certain to have existed, and that when the softening and redness exist without thickening, this is still probable; it is not, by any means, probable when the softening is found under different circumstances, that is to say, without redness and without thickening. Any other view of the subject appears to me incorrect, until it be proved that nature has only one mode of producing the softening of which we are speaking, and the contrary seems to me positively established with regard to the softening with diminished thickness of the mucous membrane of the stomach, and of the corresponding cellular tissue; as we have stated previously, and, as we shall see shortly, this is the case in other organs, in many cases. I am far from admitting, therefore, that the softening of the mucous membrane of the small intestine is always inflammatory; on the contrary, it seems to me necessary to admit that it is of an entirely different character in certain persons."

Again, in connection with the above, he says (Vol. 1, p. 170), "One of these, redness, thickening, or softening, is not, when alone, sufficient to prove that inflammation existed."

Compare these quotations from Louis's works with what Dr. Paine says of Louis in the quotation above, viz., that "the inductions rest chiefly upon the fact that the foregoing alterations of structure are white in one case and red in the other."

Thus we believe that we have refuted the second of our commentator's statements. It is very singular, but almost constantly the grossest misconceptions and misstatements made by our commentator in regard to M. Louis's views, we are able to contradict from some personal conversation we have had with the French physician. Usually it would be scarcely worth while to mention these incidents; but we cannot forbear stating the following, as it is so directly connected with our subject. We had read a paper in his presence, and in describing the state of the mucous membrane, we stated merely the *color*. Louis, with his characteristic frankness, declared "that *color alone indicated nothing*—we ought to have described the thickness, consistence, &c., of the part, and therefore our description was really of no kind of value."

But we would ask whether Dr. Paine, learned as he is, is really willing to say that softening must always be the effect of inflammation? Let him read John Hunter and Dr. Carswell, and he will have proof enough to the contrary; and he will find that softening of mucous membranes is not an uncommon result of causes wholly the reverse of inflammation.

[We unexpectedly find that the remainder must be deferred till next week.]

## TREATMENT OF DISEASES BY COLD WATER.

THE following remarks are from the Philadelphia Medical Examiner, and are introductory to some extracts from a new work, entitled "Manual of Hydrosudophy, or the Treatment of Diseases by Cold Water, Sweating, Exercise and Regimen; according to the method employed by V. Priessnitz at Graeffenberg. By Dr. Bigel.

"Within a few years past a novel method of treating disease has attained considerable notoriety in Germany, and may be regarded as the successor of Homœopathy. It is the treatment of all diseases by cold water. It matters little what the disease may be, the same curative method is applicable to it, with some little variation in the mode of its administration, but still the essential agent is the same—that is, pure and cold water.

"In acute diseases, cold water has been long used as a powerful adjuvant to other methods of treatment; rarely, however, has it been resorted to without the agency of some more powerful and more regularly admitted pharmaceutical agent; the new method is, therefore, little more than an extension of what was previously known. But in chronic diseases the case is different; water is not often resorted to, except in the form of mineral water, or of some ptisans which contain but a small proportion of medicinal substances, compared with the whole bulk of the remedies;—in these cases the active part of the remedy is of course the water. By the new method no medicinal substance whatever is used: the treatment is conducted simply by cold water, used both internally and externally, and profuse sweating. The sweats are not produced by warm drinks, or by any artificial heat, but by ablation with cold water, and afterwards by immersion in a cold plunging bath.

"Many local affections are treated by compresses wrung out of cold water, or by the spout-bath; while the same agent is used in a different way as a revulsive—that is, by immersing the feet in the water, and covering the vessel closely, so that the water becomes gradually heated, and acts as a warm pediluvium.

"A method of treatment of this nature is necessarily most powerful, and is attended with too much trouble and positive suffering to be largely resorted to after the novelty of its introduction is past. Thus far it has met with much favor; it is a most powerful alterative, and has probably been used with more discretion than will be practicable if it should be introduced into general practice. That the treatment must either do much good or harm is obvious: the thorough saturation of the system, with the copious draughts of water which are forced out at every pore by the profuse sweating, renews, as it were, the whole body, and gives a new impetus to the function of nutrition. Hence, in many chronic, and even acute diseases, which are scarcely to be reached by ordinary medication, we may find the results equally unexpected and gratifying.

"In our practice, we are in the habit of using cold water, both internally and externally, to a much greater extent than any of our professional brethren whose practice we have seen: and although we have

never ventured to recommend a plan of treatment which even approaches the severe regimen of the hydrosudopathy, we are not surprised at its results; for we have often succeeded in curing chronic diseases which had exhausted the *materia medica*, by the free use of cold water. It is true we have rarely combined it with sweating; and we have not, as a general rule, given it internally with as much freedom as we could desire, as the majority of patients submit much more readily to affusions and baths, than consent to drench themselves with the abundant draughts necessary to produce the full alterative effects of the water. The boldness of the peasant Priessnitz may lead to a more extended use of the simple remedy which nature supplies with so much liberality, while a correct examination of its powers and dangers may restrict it within the limits in which it is of undoubted utility, and free from mischievous consequences."

It seems that in diet Priessnitz is not so abstemious as some of the professed friends of cold water in this country; as from a description of his establishment, by M. Gross, we find that at dinner there were served "soup, beef, veal, mutton, pork, poultry, with salad, and, what particularly struck me, enormous cucumbers, cooked with salt, which are here the order of the day. No other vegetables but cabbage and sourcrot; fresh butter for dessert. Every one at table drinks a great deal of fresh water; from twenty to thirty glasses a day are the ordinary amount."

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## BOSTON MEDICAL AND SURGICAL JOURNAL.

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BOSTON, SEPTEMBER 16, 1840.

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### MEDICAL SCIENCE IN THE VALLEY OF THE MISSISSIPPI.

A CORRESPONDENT writes that the removal of Drs. Drake and Gross from Cincinnati to Louisville, Ky., makes a perceptible change in the medical character of the former city. He apprehends that Louisville is to be the point of medical attraction, unless Lexington prevents it by counter efforts. An intimation is thrown out that the physicians of Louisville intend making a stir before the next Legislature, in relation to the school, which seems not to be particularly a favorite with them.—We are also informed that the rhinoplastic operation spoken of heretofore, as having been successfully performed at Cincinnati, turned out not to be entirely satisfactory, and it has been repeated more than once on the same patient.

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*Medical Lectures in Baltimore.*—A correspondent writes from Baltimore that Prof. Hall delivered a very interesting discourse, Sept. 6th, and represents him to be particularly well qualified for the chair of Obstetrics, Hygiene and Medical Jurisprudence. Dr. Smith's introductory is also well spoken of. It seems by this, that the lectures in the University of Maryland have commenced this season much earlier than usual. The Medical Almanac says that the annual course commences on the *last Monday of October*—and the information was derived from the circular of last

year. "You will see," says the letter, "that this institution has been greatly depressed, but she seems to be rising again. The prospect now is quite flattering. There is a spice of jealousy between the two schools in this city."—A considerable number of cases of erysipelas are spoken of as existing in Baltimore at the present time.

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*Lectures on Medical Jurisprudence.*—Our old and long-trying friend, Stephen W. Williams, M.D., is about publishing an abstract of his instructive lectures, ostensibly for the use of students. Having been many years in collecting the most ample materials, he cannot fail of being successful in the undertaking. About a dollar book is contemplated, which will soon be in readiness for the printer. Dr. Williams holds the chair of Medical Jurisprudence in two or three medical schools, we believe, at the present time.

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*Review of Dr. Gross.*—The manuscript of a critical analysis of the late work by this gentleman, has been received, and will have an insertion as soon as a few other papers which have been some time on the table, are disposed of. The effect of these critical examinations has an important influence, which is felt through all the avenues of authorship, and we are gratified, therefore, to perceive that the number of close readers and able critics of native medical books, is certainly increasing.

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*Fairfield Medical School.*—It is reported that the faculty have wholly abandoned the idea of giving a course of lectures at Fairfield the present season. No very strong reason has been given for this resolution. Formerly it was a very flourishing country medical institution, where many excellent physicians and surgeons were educated.

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*Dieffenbach on the Division of Tendons.*—In the treatment of torticollis, M. Dieffenbach sometimes divides the sternal portion, sometimes the clavicular portion, and occasionally both origins of the sterno-cleido-mastoid muscle, when shortened. If the patient be young, the head immediately becomes straight after the operation, and a cure is obtained in a few days; and, in most cases, the lateral curvatures of the spine, which accompany torticollis, disappear gradually. The section of the muscles and tendons connected with the knee-joint, has been frequently performed by M. Dieffenbach with the greatest success; in one case, that of a boy ten years of age, the joint was so much contracted that the heel was in contact with the buttocks. In the more complex and difficult cases, which are often classed together under the name of *spontaneous dislocation* of the hip, very great benefit has been derived from the section of the shortened muscles or their tendons. Contractions of the upper extremities, and of the fingers from rheumatic or gouty affections, were frequently cured by division of the tendons; and M. Dieffenbach remarks that these deformities are generally cured with much greater facility, than analogous lesions of the lower limbs. In two patients, laboring under organic disease of the brain, which had occasioned permanent flexion of the forearm, with spasmodic contraction of the fingers on the palm of the hand, M. Dieffenbach divided the tendon of the biceps, flexor carpi radialis, flexor carpi ulnaris and flexor digitorum. A remarkable improvement was obtained, and the

patients were enabled to grasp and hold objects of a certain size. Finally, in 400 operations which have been performed by the author, he has never seen any nervous accidents, hæmorrhage, or troublesome suppuration, produced by sub-cutaneous division of the tendons.—*London Lancet*.

*On the External use of Living Ants (formica rufa).* By Dr. SCHREIBER, of Russia.—The summer division of the author's hospital lies in a wood, where there are so many ant-hills, that the thought struck him of drawing some advantage from them for his patients. As ant-baths and tincture of ants were of no great use, he tried the living insects in paralysis, hemiplegia, paresis and inveterate arthritis. The ants are to be taken directly from their hill, and put in a bag; and this bag is to be tied over the limb in such a manner that the ants cannot escape (but obtain access to the skin). Some time after their application to the paralyzed limb, the patient begins to feel the running and biting of the ants, by which they gradually excite a kind of electrical twitches, and a feeling of warmth, which gradually extends over the whole body. Moreover, by their ethereal principle, they cause as violent a perspiration over the whole body, as if the patient were in a vapor bath. The paralyzed part must be kept in the bag with the ants for two or three days; the patient is then to rest for a day, after which the ants are to be applied again; and this is to be repeated till the object is attained. In 1835, Dr. Schreiber obtained a favorable result in seven cases of paralysis; in 1836, in four; and in 1837, in three; by which he was encouraged to use the same remedy in chronic rheumatism and gout. It is unnecessary to remark that this remedy alone cannot be of much service, if the case is complicated with syphilis, scurvy or scrofula; but the military hospitals have plenty of uncomplicated cases, and in three years 46 patients under this head were cured.

Dr. Schreiber now began to use the remedy in dropsy proceeding from inactivity of the skin. In anasarca it was found sufficient to tie up the lower extremities in bags with living ants, and thus obtain profuse sweating and a cure. This method of treatment, supported by gentle purgatives and sudorifics, succeeded in 24 cases.—*Lon. Med. Gaz.*

*Nasal Polypus cured with Sanguinaria Canadensis.*—Being lately in Newark, Ohio, Dr. Brice, for more than thirty years a respectable practitioner of that place, narrated to us three cases of polypus of the nostril, which he had permanently cured by the application of the root of the *sanguinaria canadensis*. One of the patients was a youth, in whom the polypus projected out of the nostril. A physician in a neighboring town tore away a part or the whole of it, and the operation was followed by a profuse hæmorrhage. Some time afterwards the doctor saw him, and the polypus again extended beyond the *alæ nasi*. The application of the powdered root and the decoction of the *sanguinaria* soon caused it to assume a pale color and shrink up. Under the continued use of the medicine he entirely recovered.

Another patient was a little girl in whom the polypus was distinctly seen, but it did not present itself entirely. The same applications effected a radical cure.

A third was a man rather advanced in life, whose nose was much obstructed by the size of the polypus, but it did not descend to the lip. It was permanently removed by the same treatment.

We do not recollect to what extent the sanguinaria has been employed in the treatment of polypus, and are writing these memoranda remote from all books of reference. Should the reader be already familiar with the use of this remedy, he cannot charge us with prolixity in this testimony of its efficacy.—*Western Jour. of Med. and Surg.*

**Operation for Strabismus.**—We learn that Dieffenbach's operation for strabismus has been performed by Dr. Hays, of Philadelphia, in three cases, and promises to be successful. D. H. states that the immediate effects have been exaggerated, but that an important benefit results in the great improvement of vision, which is not mentioned by foreign operators.

Number of deaths in Boston for the week ending Sept. 12, 53.—Males, 22—Females, 31. Stillborn, 2. Of consumption, 4—dysentery, 2—cancer in the bowels, 2—infantile, 5—dropsy on the brain, 1—typhoid fever, 2—hooping cough, 2—lung fever, 4—diarrhea, 1—cancer, 1—intemperance, 2—cholera infantum, 4—cancer in the womb, 1—delirium tremens, 1—cancer on the lungs, 1—cancer, 1—old age, 1—teething, 2—hydrocephalus, 1—paralysis, 1—cramp, 1—guinea, 1—cholera morbus, 1—rheumatic fever, 1—scarlet fever, 1—dropsy, 2—inflammation of the bowels, 1—bowel complaint, 1—scalds, 1.

# MASSACHUSETTS MEDICAL SOCIETY.

A STATED MEETING of the Councilors of the Society will be held at their rooms, rear of the Athenaeum, Pearl Street, on Wednesday, the 7th day of October next, at 11, A. M.  
S 16—tm GEO. W. OTIS, Jr., Sec. Soc'y.

# MEDICAL TUITION FOR 1840—41.

THE subscribers will commence their course of instruction for the ensuing medical year, on November 1st, 1840 (the period at which the Lectures at the Medical College of Harvard University begin). Minute examinations will be held on all the branches of medicine and surgery during the lectures, in order that students intending to offer themselves for examination at the College in the spring, may be prepared. Students may be assured that they will have constant and abundant opportunities for the cultivation of practical anatomy at all seasons of the year. After the lectures, the arrangements will be as follows until the ensuing November.

Free access at all hours to the United States Marine Hospital at Chelsea will be granted; a daily morning visit will be made by Dr. Stedman, and every week Drs. Perry, Bowditch and Wiley will visit in the afternoon, for the purpose, chiefly, of learning the physical signs of diseases of the chest. Dr. Bowditch will deliver a course of lectures on diseases of the chest and air passages. Admission to the medical and surgical practice at the Massachusetts General Hospital, the Infirmary for Diseases of the Lungs, and to the practice of one of the Dispensary Districts; occasional opportunities for operative surgery and midwifery.

Courses of instruction as follows:

Theory and Practice of Medicine and Chemistry, by	DR. PERRY.
Midwifery, Materia Medica and Demonstrations on }	
Morbid Anatomy at the Hospitals, by	DR. BOWDITCH.
Anatomy, Surgery and Medical Jurisprudence, by	DR. WILEY.
Rooms for study either at Boston, at the Infirmary for Diseases of the Lungs, or at Chelsea, free of expense. For terms, apply to H. G. Wiley, M.D., or to either of the subscribers.	
DR. PERRY, 412 Washington st.,	DR. STEDMAN, Chelsea Marine Hospital,
DR. BOWDITCH, 6 Otis Place,	DR. WILEY, 467 Washington st.
S. 16—sept.	

# JEFFERSON MEDICAL COLLEGE OF PHILADELPHIA.

THE regular Lectures will commence on the first Monday of November.  
The following are the professors, in the order of their appointment:—

1. JACOB GREEN, M.D., Chemistry.
2. GRANVILLE S. PATTISON, M.D., Anatomy.
3. JOHN REVERE, M.D., Practice of Medicine.
4. ROSELY DUNGLISON, M.D., Institutes of Medicine and Materia Medica.
5. ROBERT M. HUSTON, M.D., Obstetrics and Diseases of Women and Children.
6. JOSEPH FANCAUST, M.D., Surgery.

On and after the 1st of October the dissecting rooms will be kept open, and the Professor of Anatomy will give his personal attendance thereto. Lectures will likewise be delivered regularly during the month on various branches, and opportunities for clinical instruction will be afforded at the Philadelphia Hospital under the Professors of Medicine and Surgery; and at the Dispensary of the College under the Professors of Physic and Surgery.

Philadelphia, July 15, 1840.

A. 26.—1N1

JOHN REVERE, M.D.,  
Dean of the Faculty.

# ORDERS FOR GOODS FROM PARIS.

THE subscriber intending to remain in Paris for a year or two, will be happy to attend to any orders, of large or small amount, for physicians or others, who may be in want of books, instruments, minerals, &c. &c. Reference to Dr. Martin Gay, and Nathan Hale, Esq. Directions may be left with Mr. Foster, at the Courier Office, 4 Thorndike's building, Congress square, Boston.

Sept. 5.

S 16—St

J. H. BUCKINGHAM.

## BOYLSTON MEDICAL PRIZE QUESTIONS.

The Boylston Medical Committee, appointed by the President and Fellows of Harvard University, consists of the following physicians, viz.:

JOHN C. WARREN, M.D.	JACOB BIGELOW, M.D.	JOHN RANDALL, M.D.
RUFUS WHEAT, M.D.	WALTER CHANNING, M.D.	ENOCH HALE, M.D.
GEORGE C. BRATTUCK, M.D.	GEOFFREY HAYWARD, M.D.	JOHN WARR, M.D.

At the annual meeting of the Committee on Wednesday, August 5th, 1880, the Boylston premium of fifty dollars value was awarded to W. W. Gerhard, M.D., of Philadelphia, for a dissertation on "the pathology and treatment of typhus and typhoid fever," with the motto, "Je sais que la verité est dans les choses, et non dans mon esprit que les juges." The other Boylston premium of the same value was adjudged to Joseph Sargent, M.D., of Worcester, Mass., for a dissertation on "the pathology and treatment of medullary sarcoma," with the motto, "On observe la nature; on ne la devine pas."

The following prize questions for 1881, are already before the public, viz.: 1st. "To what extent is disease the effect of changes in the chemical or vital properties of the blood?" 2d. "The structure and diseases of the teeth; with a numerical solution of the question, can caries of the teeth be retarded by mechanical processes?"

Dissertations on these subjects must be transmitted, post paid, to John C. Warren, M.D., Boston, on or before the first Wednesday of April, 1881.

The following questions are offered for 1882. 1st. To what extent is the human system protected from smallpox, by inoculation with the cowpox? Is the protection increased by re-vaccination; and if so, under what circumstances? 2d. On the diseases of the kidney, and the changes which occur in the appearance and composition of the urine, in health and in disease.

Dissertations on these questions must be transmitted as above, on or before the first Wednesday of April, 1882.

The author of the best dissertation on either of the above subjects, will be entitled to a premium of fifty dollars, or a gold medal of that value, at his option.

Each dissertation must be accompanied by a sealed packet, on which shall be written some device or sentence, and within shall be enclosed the author's name and residence. The same device or sentence is to be written on the dissertation to which the packet is attached.

All unsuccessful dissertations are deposited with the Secretary, from whom they may be obtained, if called for within one year after they have been received.

By an order adopted in 1886, the Secretary was directed to publish annually the following votes, viz.:

1st. That the Board do not consider themselves as approving the doctrines contained in any of the dissertations to which the premiums may be adjudged.

2d. That in case of the publication of a successful dissertation, the author be considered as bound to print the above vote in connection therewith.

ENOCH HALE, Secretary.

Publishers of newspapers and medical journals, throughout the United States, are respectfully requested to insert the above notices.

Boston, Aug. 6, 1880.

A 12.—4t

## MEDICAL INSTITUTION OF YALE COLLEGE.

This annual course of Lectures, for the term of 1880-1, will commence on Thursday, October 1, and continue sixteen weeks.

Chemistry and Pharmacy, by	BENJAMIN SILLIMAN, M.D. LL.D.
Theory and Practice of Physic, by	ELI IVER, M.D.
Materia Medica and Therapeutics, by	WILLIAM TULLY, M.D.
Principles and Practice of Surgery, by	JOSIAH KNIGHT, M.D.
Obstetrics, by	TIMOTHY F. BREE, M.D.
Anatomy and Physiology, by	CHARLES HOOKER, M.D.

Fees for a full course, \$76, to be paid in advance. No dissection fee is required, nor any contingent expenses, except a reasonable charge for subjects, which are abundantly supplied.

Yale College, New Haven, July 17, 1880.

Jy 29—4t

CHARLES HOOKER, Sec'y.

## MEDICAL LECTURES IN BOSTON.

The Medical Faculty of Harvard University will begin their annual course of Lectures on the first Wednesday of November next, at the Massachusetts Medical College, Mason street, Boston. The Introductory Lecture will be given at 12 o'clock, M., in the Anatomical Theatre, on that day, and the lectures will continue four months.

Anatomy and the Operations in Surgery, by	Prof. WARREN.
Midwifery and Medical Jurisprudence, by	Prof. CHANNING.
Materia Medica and Clinical Medicine, by	Prof. BIGELOW.
Principles of Surgery and Clinical Surgery, by	Prof. HAYWARD.
Chemistry, by	Prof. WEBSTER.
Theory and Practice of Physic, by	Prof. WARR.

The students will have an opportunity of attending the medical and surgical practice at the Massachusetts General Hospital, and also of seeing the surgical operations performed there during the winter.

The Faculty have reason to believe that the provisions of the law legalizing the study of anatomy, will be carried more completely into operation than has heretofore been done, and that the facilities for practical anatomy will consequently be much increased.

WALTER CHANNING, Dean.

Boston, July 6, 1880.

Jy 15—4t

THE BOSTON MEDICAL AND SURGICAL JOURNAL is published every Wednesday, by D. CLAPP, JR., at 184 Washington St., corner of Franklin St., to whom all communications must be addressed, post paid. It is also published in Monthly Parts, with a printed cover. There are two volumes each year. J. V. C. SMITH, M.D., Editor. Price \$3.50 a year in advance, \$3.50 after three months, or \$4.00 if not paid within the year. Two copies to the same address, for \$5.00 a year in advance. Orders from a distance must be accompanied by payment in advance or satisfactory reference. Postage the same as for a newspaper.

*At the Medical*